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THE  
ANTHROPOLOGICAL REVIEW.

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AN INQUIRY INTO CONSANGUINEOUS MARRIAGES AND  
PURE RACES.\*

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TRANSLATED BY HUGH J. C. BEAVAN, F.R.G.S., F.A.S.L.

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I. PRELIMINARY REMARKS.

BEFORE entering into an analysis of the facts upon which depends the theory attributing serious inconveniences and numberless dangers to posterity from marriages between cousins, allow me to offer to your notice an essay on this question which I published in July, 1862, entitled "*Des dangers attribués aux mariages consanguins.*" Since the appearance of this treatise I have not appeared before the Society, and the question has never been mooted at its meetings. M. Devay, whose loss we all deplore, was thus deprived of an opportunity of presenting to the Society an answer which he had made to my work: "*Un mot sur les mariages consanguins, réponse à une attaque.*" It seemed, then, that the question of consanguineous marriages, so complex, so extensive, would not be discussed before you for a long period; but since M. Boudin, in the last number of the *Memoirs of the Society*, has published a fifth edition of his work, "*Sur les dangers des unions consanguines,*" it obliges me to communicate to you at once the result of the inquiries I undertook more than a year ago, and which I intended to bring before your notice at some more distant period.

If I thus anticipate the time when my researches would have been complete, it is not, indeed, because an answer has been made to my

\* This paper was read before the Anthropological Society of Paris by Dr. E. Dally, November 5th, 1863. (Tr.)

former objections. But time passes, and statements of M. Boudin are continually appearing, without opposition, without criticism, in journals, in reviews, and as essays; in all of which he declares it is impossible to resist the conclusions of our honourable colleague. Besides, as in some notes in which my name appears, a semblance of an answer has been made to my former work, some persons may believe that such an answer has really been made. It is with the intention of combating these two tendencies that I take the liberty of submitting to you to-day my researches, sufficient, I believe, to contest with the disciples of M. Boudin a triumph as blatant as it has been easy. That the readers of M. Boudin's writings, not being able to examine into all they read, and especially the statistics and extracts, have been struck with the mathematical precision of his assertions, and the eloquence of his remarks, can be easily understood; but that literary men, critics, and above all, those who have written on the subject on their own account, should not have given themselves the trouble to examine the documents furnished by M. Boudin, and that in the presence of a contradictory work they have reproduced without any further explanations the same documents, the same notes, the same conclusions, is a thing which certainly does no honour to the criticism of the age. Allow me to explain the present state of the discussion.

No one, I said, has, up to the present time, answered the objections contained in my work. Messrs. Devay, Boudin, and others have occupied themselves a little about me, certainly, but I have no longer the right to refute the essay—rather too personal, perhaps—published by M. Devay, and M. Boudin has restricted himself in his remarks to the criticism of comparatively unimportant details. In the first of these remarks he accuses me of having produced “neither a fact nor a statistic.” It was not my business to do so. I had not to sustain a doctrine, but to examine that which was already produced. Now, neither the documents which have been exhibited, nor the methods which have been employed as a means of giving them sense, have appeared to me to offer the necessary guarantee for the introduction into physiology and legislation of a doctrine which might be made the subject of domestic dissensions and of individual evil. Things being as they are, it is for those who desire to modify them to give motives worthy of their projects. Besides, I could, long ago, have produced both facts and statistics, but it is not in the face of attacks of which I am the subject that I choose to resume this question, which would prevent me from attending to other work.

The four notes in the review of our colleague's work, "Sur le croisement des familles," will be answered at some later time: they do not in the slightest concern the objections which I have had the honour to present to him, and which I resume now as follows:—

1. In the first edition of M. Boudin's essay, the total number of inmates in the Deaf and Dumb Institution of Paris was not declared. It was incorrectly given in our own *Mémoires*.

2. In the same edition we read that "the resemblance of the proportional numbers found by MM. Landes, Chazarain, and Boudin, constitute a very powerful argument in favour of the precision of their observations", which tended to admit the influence of consanguinity in marriage upon the production of deafness and dumbness. In fact, M. Boudin had deduced from a few observations made in the departments of the Rhône and the Gironde, that in every 100 deaf and dumb cases, he discovered that at Bordeaux 30 per cent., at Lyons 25 per cent., and at Paris 28 per cent., were of consanguineous origin. This uniformity would, indeed, be significant if the number of the deaf and dumb was at all equal in the above named departments. But this number is very different, as it happens; there is in the Seine 1 deaf and dumb case to 4,694 inhabitants; in the Rhône, 1 in 1,669; in the Gironde, 1 in 1638. Now if marriages between cousins have some influence upon the number of the deaf and dumb, the marriages ought to be more frequent in the Rhône than the Seine, and reciprocally, the number of the deaf and dumb ought to be raised wherever there are many consanguineous marriages. In other terms, if there are three times more deaf-and-dumb cases in the Rhône than in the Seine, the proportion of the deaf-and-dumb of consanguineous origin to the deaf-and-dumb of every origin ought to be three times more considerable at Lyons than at Paris. Very well; if at Paris M. Boudin has found 28 per cent., he ought to find in the Rhône about 84 per cent. It would have been easy to answer to this reasoning by facts which would have confirmed or nullified it, and to establish that there are in the department of the Rhône two-and-a-half times more consanguineous marriages than there are in that of the Seine. They preferred, however, to renounce the argument drawn from the resemblance of statistics.

3. I had proposed a plan which consisted in making inquiries *by departments*, and in comparing reports far more complete than those of which M. Boudin has made use. Neither M. Devay, nor M. Boudin, nor their pupils, have once mentioned this.

4. I had foreseen that the number of marriages officially declared

and registered between first cousins was below the truth, and we shall see that this argument was correct, since (in certain communes at least) no mention is made of the degree of cousin in the registers, and in many others no statistical information has been furnished on this point. It followed that the declared percentage of consanguineous marriages—2 per cent.—was too small, and that the true number was not known even to those best informed on the subject. Nevertheless, M. Boudin maintains that his statistics are correct.

5. I had stated that to bring forward the danger of procreating deaf-and-dumb children by marriages between nephews and aunts—overstepped by 70 per cent. the most extensive limits of inference, the proof lying merely in one case of deaf-dumbness from this origin in the Institution at Paris. There was no answer to this. It is by an analogous inference that they pretend to find only one half-bred Jew deaf and dumb. It is evidently impossible to form any conclusions from such statistics.

6. Now, some apocryphal documents having been produced in the course of discussion (statistics from Ohio and Massachusetts), I have felt it my duty to bring forward this fact, and thus I have prevented future compilers from making many serious errors. They have not given me credit for it. Perhaps they will this time tell me whence comes the document attributed to Don Ramon de la Sagra, and whence come the statistics of Mr. Morris, who has studied the cases of 4,013 children of consanguineous origin? Who is Mr. Morris? and where has he published the experiences of these extraordinary inquiries, which must have cost him so many long years of study? This document must be looked upon with a great deal of suspicion when we find that one of those authors who copies M. Boudin literally—even in his mistakes—has abstained from copying it.

I am, therefore, authorised to reiterate my previous criticisms; and, upon some points, I expect further to unfold them. Allow me, before I do so, to bring the subject before your notice in what I believe to be a true light, and to eliminate from the discussion certain foreign elements; allow me, in other terms, to bring forward the question itself.

## II. THE LIMITS OF THE SUBJECT.

The considerations which follow, I believe, must be disregarded with reference to animals and vegetables. For, besides the convenience of allowing certain men to examine into special questions, the observations made on animals do not appear exactly applicable

to mankind, at least so far as regards "pairing"; for the conditions of existence are completely different, and the aim which is proposed in zoology has no analogy in social life. It is, therefore, difficult to avoid the unlucky confusion which is continually being made between "selection" and consanguinity, which is only one of the results of selection; this result, applied to realise a known end, cannot be assimilated by factitious consequences with the spontaneous results of a consanguineous union among mankind. It may happen, in fact, that when consanguinity is applied to animals for the purpose of selection, it produces an artificial development of some parts of the animal, and may thus injure the *general* development. But this result is not avoided when we practise selection, beyond all consanguinity. It is not, therefore, logical to attribute to consanguinity that which is, or may be, attributable to selection.

Also, if breeding in-and-in (*sic*), that is to say, consanguinity increased a hundred-fold, had not, perhaps, given in its interior action the magnificent results which have been proved by the zoologists (and which our colleague, M. Sanson, has clearly shown, with his usual talent), we have no right to extend to mankind, by pure induction, the laws obtained from observation of the domestic animals. Equally, if it were established that the consanguinity of human unions, so far from showing the dangers which our opponents give it, is in the end advantageous, what shall we say about a theory which, without experimental data, without taking account of the respective differences of man in his social state and animals (that is to say, the liberty necessary for human development, and the subjugation not less necessary for the development of animals for the use of man), would proclaim at once the necessity of consanguineous unions?

For this reason, I deprive myself without regret of the support which those zoologists could offer me, who have, in my opinion, out-run anthropologists, and marked, not without precision, the degree in which the consanguinity of reproducers is useful to selection. As to the vegetable kingdom, I do not think it will be proper to bring before your notice the curious connection of relationship which some have tried to prove—with more boldness than good sense—between the fecundity of plants and animals, with reference to consanguinity. If, besides, this same theory could bear the slightest examination, our opponents would not be able to find in it any arguments favourable to their thesis; but of what use is it to extend, so far as the subject of such a strange assimilation, a question which it is above all things necessary to narrow, confine, and specialise?

Do they believe that the definitions may be, as regards mankind, so clear, and that the problem is so well stated, that facts alone may henceforth solve it? That would be a fatal error. The method and its interpretation play here a part of the first order. They speak of consanguineous marriages; they reprove them, and wish them to be forbidden. The question being proposed in these general terms, who can hesitate a single moment in giving his support to the reproof and the interdict? For does not the term "consanguineous marriage" comprehend incestuous unions of all degrees? And how is it that, in order to perpetuate the confusion, our adversaries do not fill up their writings by the documents—more or less authentic—in which are shewn, on the strength of the interdiction of marriages of distant kindred, the moral and physiological evils of incestuous marriages in barbarous or savage countries?

It is painful to bring this remark before your notice, it seems so very puerile; and yet it is necessary to re-establish the thread of the discussion in which we are now engaged. It refers solely to the dangers attributed to marriages *between cousins*; beyond that, it does not even refer to the *moral* inconveniences of such unions. Whatever opinion there may be on this point, it will not figure in our debate; nevertheless, not because morality ought to be separate from physiology, but because it is necessary to simplify a question which is already only too complicated of itself. If *moralists* can see the chief inconveniences of unions between cousins—and such were the first fathers of the Church—by all means let such alliances be forbidden; but let it be done then for moral or theological motives, and not for reasons borrowed from a false biology. Such is, however, the origin of this discussion. The civil law has given such force to canonical prescription fallen into disuse, that dispensations are never refused. One would wish, however, to restore these prescriptions, by relying not only on the wise and legitimate reasons of their authors (since these reasons, as we shall see later, no longer exist), but by relying on statistics and physiology. Another consideration, entirely theoretical, must be brought forward. I will suppose, then, that marriages between cousins furnish really a larger proportion of weakly and sickly children than other marriages; this fact, well established, may receive a certain number of rational explanations; but, doing away with this idea that every man is affected with a morbid predisposition, more or less developed, and that these predispositions have a tendency to perpetuate themselves in all the branches of a family, one may fear that two first cousins, affected with the same predisposition,

should transmit it to their children in a stronger manner than it appeared in themselves; each of these two individuals being slightly gouty, for instance, would produce a child who from his youth would show symptoms or injuries which would prove the existence of an uric diathesis of the worst description. Now I know not if such is, in reality, the law of morbid transmissions; it must be controlled by facts, and this part of the work has not yet been done. But, in all cases, the affirmative hypothesis has nothing in it which offends reason; on the contrary: and I know there are many hygienists who are hostile to consanguineous marriages on account of this very plausible theory.

Such is not the position of our opponents. They maintain that consanguinity, *ipso facto*, pure consanguinity, has *of itself*, in the absence of all disease in the parents, the property of producing diseases in the children. It is only this theory which I am now attacking, without caring to know if the facts which I am examining confirm or weaken the question of the dangers of a doubly unhealthy consanguinity.

Upon this last point, nevertheless, I am not prepared to think that the chances of disease in children are more numerous when the two parents are afflicted with the same disease, than when they show, each one separately, special predispositions; for, if we suppose that the laws of morbid inheritance are invariable, it would follow in the first case that the child would be affected with a predisposition in some degree *pure*; in the second with a predisposition, so to speak, *hybrid*; and, all things being equal, I believe that clear and well characterised diseases are less rebellious to therapeutics than those in which all sorts of pathological *elements* are mingled. This would require to be scientifically established or refuted; but the dangers of consanguineous marriages, at all events in the way in which they are understood by Messrs. Devay and Boudin, have nothing common to inheritance. M. Boudin insists strongly on this point. "In our opinion," he says, "consanguineous marriages, so far from militating in favour of an entirely imaginary and morbid inheritance, constitute the most powerful argument against the laws themselves of inheritance. Why, you see parents who are consanguineous, full of strength and health, exempt from all appreciable disease, incapable of giving to their children the health which they themselves have—giving them, on the contrary, that which they do not themselves possess; and it is in presence of such facts, that some persons dare to bring forward the words 'morbid inheritance!'" Assuredly, the audacity must be great



which enables any one to say this. Is it less audacious to say that it is consanguinity *ipso facto* which is the cause of it?

In the case of two persons not closely related and in good health, who have weakly children, the cause of this weakness escapes your observation altogether: it is by this, then, that we are advised to declare that these facts protest against the laws of inheritance. Do we dare, for want of anything better, to take one particular circumstance in the case of individuals, and then to say, "you are not discovering the cause of this disease, behold it here"?

To sum up; we must exclude from anthropological researches on consanguinity all facts which relate to plants and animals; we must only study legitimate marriages between cousins or collaterals, and not between direct progenitors and relations in the first degree; we must disengage from this study all considerations of domestic and moral order: we must see, then, if the facts which are produced confirm or weaken the theory of the dangers of healthy, in opposition to morbid, consanguinity. These are two questions, connected, yet distinct. Such are, let us consider, the first conditions of every controversy.

I will, therefore, examine successively. 1. The statistics which have been made in the asylums for the deaf-and-dumb, for the purpose of proving that consanguineous marriages furnish these asylums with a relatively larger proportion of inmates. 2. Observations comprising particular cases observed by French or foreign physicians, taken from a large number of patients. 3. Assertions regarding the decay of the higher classes, and the comparative value of races called pure, and especially of the Jews. 4. Facts which are favourable to the practice of healthy consanguinity. 5. Historical documents which have reference to the laws of antiquity, and which touch on the origin of the illegality of consanguineous unions.

### III. STATISTICS CONCERNING THE DEAF-AND-DUMB IN ASYLUMS.

Our honourable colleague, M. Boudin, had an excellent plan for relieving the Paris asylum of the number of patients who owe their birth to consanguineous parents. He has been led by this means to compare the proportion of deaf-and-dumb cases of this origin, with the proportion of marriages between relations, and the difference between these two products has given him a proximate value of the dangers which these unions present. If, in fact, there are 5 consanguineous marriages in 100, one ought consequently to find 5 cases of deaf-and-dumb children of *consanguineous origin* in every 100 cases of the same nature.

But in order that this plan may lead us to undeniable results, we

must certainly have both the record of all consanguineous marriages, and that of all the deaf-and-dumb. We must, besides, in order to avoid the chances of exception, be able to compare the number of deaf-and-dumb with the total population of a given place, and, in fact, to be able to compare the statistics of many other places with them. If we arrive, by this means, at analogous results, we should certainly have the right to consider them as finally acquired: the case would be so, for example, if this work had been done separately in each department of France. In my first notice on the subject I sketched a plan which M. Uytterhœven proposed last year at the Social Science Congress at Ghent, as the foundation of a debate which should have taken place there this year.\* But the question of consanguineous marriages, already so extensive, having given place to the indefinite one of "Civil Marriage and its Consequences," I have had the sorrow of learning that the question never touched on our subject, and that no statistics whatsoever were produced.

The difficulties which one experiences in obtaining exact reports are besides very considerable, and have prevented many persons who promised me their impartial assistance from giving effect to this promise. In fact, if it is easy by the table of exceptional cases, and the general register of marriages to recognise the number of adult male deaf-and-dumb cases of a department, it is almost impossible to have the register of the deaf-and-dumb of every age and sex, and that of marriages between cousins.

The regulations of the *préfecture* prescribe the registration of marriages between first cousins, uncles and nieces, nephews and aunts, sisters-in-law and brothers-in-law; but these registrations are incomplete in the towns, and entirely neglected in the communes. In the offices of the *mairie* at Paris, the statistics of marriage are registered monthly with great exactness, with the individual relationship, and it is from these that the clerks register the degree of cousinship. The future parents are not directly questioned, and their relationship is not an object of particular registration, either in the record, or the registers. One can understand, then, how many omissions must be made in the long and uninteresting work of abstracting from numerous records (1000 per month in the 8th district) a page of statistics in which is comprised thirty or forty questions.

But it is not in towns that marriages of relations are the most numerous: it is certainly in the country. Now, in a great many country places (I myself know of three communes), no account is taken of the relationship of parents, excepting in the case where legal

\* See the Report of the Meeting, p. 562, 1863.

dispensations are necessary (uncle and niece, aunt and nephew). Most people know that, in general, the communal schoolmaster fulfils the duties of secretary to the mayoralty; these *employés* have usually a manual recommended by the Minister of the Interior,\* and according to the instructions found therein they draw up their records; one can read there the enumeration of "eleven declarations common to all the records of marriage," and in these there is not a single question on the subject of relationship.

These instances alone would suffice, I think, to authorise us to consider that the official number of marriages between cousins is very much under the reality, and that they do not comprise the country population, in the centre of which the statistics are regulated. The total number of marriages is, on the contrary, rigorously exact everywhere. Hence it follows that when M. Boudin values, by official documents, the proportion of "relationship marriages" at 0.9 per cent., this number has, in my eyes, notwithstanding its official origin, no scientific value whatsoever, because I know that it rests upon the authority of incomplete data. And that which was at first my opinion only is become a certainty, since I inquired of M. Legoyt, the head of the statistical office of France, the manner in which the numbers published by M. Boudin had been obtained; this official has authorised me to declare that he cannot answer for any statistics, except those which have reference to the legal dispensations necessary for marriages between uncle and niece, aunt and nephew; so, since marriages between first cousins do not require this permission, M. Legoyt is convinced himself that the extracted numbers are incomplete, and he has prepared a circular destined to remedy the various mistakes already noticed. Future parents will be henceforward directly questioned about their relationship; mention will be made in the records of their answers, and there is reason for hoping that, in a few years, we shall learn the real proportion of consanguineous marriages. In the meanwhile I have examined, at the *mairie* of the eighth district of Paris (formerly the first), the monthly records of marriages celebrated during a period of ten years, from 1853 to 1862, and I have obtained from them the following results:—

Total number of marriages, 10,765.			
Marriages between first cousins	-	-	141
„ „ uncle and niece	-	-	8
„ „ aunt and nephew	-	-	1

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150

\* Guide du maire et du secrétaire de mairie, par M. Hallez-d'Arros, 1858.

(This number may vary from 146 to 152, on account of three figures which are uncertain.)

These numbers give us a proportion of about 1·4 per cent. And it appears to me impossible to admit otherwise than this, that, in a district of Paris which is inhabited by foreigners, showing a considerable floating population, there are many less marriages between cousins than in the midst of small towns and in the country. This is why, finding here 1·4 per cent., I am authorised to say that 0·9 seems to be three or four times too small a percentage for the whole of France.

But our criticism does not stop there. Starting from the incorrect proportion of 0·9 per cent. of marriages between first cousins, M. Boudin wishes to value the number of marriages between cousins, *children of first cousins*, so as to be able to comprise in the morbid cases, due according to him to consanguinity, those which are observed in children who are the issue of such marriages.

Unfortunately, here all the elements of statistics are completely at fault; in such a case, it is not worth while giving up a value which is necessarily arbitrary. M. Boudin does not understand it thus: he wishes to comprise in his statistics the cousins who are themselves children of first cousins (and even, as we shall see, as far as cousins of the seventh degree), and he believes that by adding 1·1 for these last he has sufficiently valued their proportion in relationship marriages: we thus obtain 2 per cent. ( $0·9 + 1·1$ ) as the number around which to group a large number of deductions. Now, as for this second fraction, M. Boudin is not more fortunate about it than he was with the first; for while he fixes at 1·1 the proportion of marriages between the children of first cousins (and others), I can myself fix it at 5, 10, or 15 per cent. We are here speaking of a matter of pure hypothesis. Every one can choose his own; and since we ought to find three or four times more children who are offspring of first cousins than first cousins themselves, my first number, however exaggerated it may seem, will be much nearer the truth than that of M. Boudin. In whatever way we regard it, it is impossible to agree with M. Boudin that marriages between first cousins are in the proportion of 2 per cent. Such is, however, the fundamental idea in our colleague's essay.

Let us now examine the other elements. M. Boudin announces that among 200 patients he found 95 who had been deaf and dumb *from birth*. I demand, therefore, why this elimination of 105 cases, and I am answered: "Would M. Dally have desired that we should

have examined into the consanguineous origin of those who have become *accidentally* deaf and dumb?" The question is certainly ludicrous; I beg pardon for it, but I must really be allowed to state that the term is here very badly applied: *accidental* is not employed in opposition to *congenital*: *acquired* is, in this case, the proper word. To call the deafness which occurs after birth *accidental* is not using the language of medicine, where, in fact, the word receives a signification quite foreign to that which now engages our attention. But for men of the world, the unscientific class, *accidental* seems to give the idea that ordnance has been fired off close to the ears of M. Boudin's 105 patients. What an absurd thing it would be to endeavour to discover the consanguineous origin of *deaf* artillerymen? This is, however, the very thing which, according to M. Boudin, I reproach him with not having done: evidently he mistakes the sense of the objection. This mistake rectified, I do not hesitate to answer "yes, I should have wished you to comprise in your list the 105 cases that you have eliminated, and a great many more;" for if, on one side, the influence of consanguinity is real, why is it not exercised as well after as before birth? Is it not so with all predispositions and nervous affections? And as to that which regards the inheritance of anomalies, is it not a rule that it shows itself at a certain age, often an advanced one? What is deaf muteness besides? Are its organic causes so well known that we can afford to pass them over in silence? Do we believe that it is an arrest of development, or do we consider that it is an injury entirely functional? Do we not know that there are deaf children who *do* hear slightly at birth, but whose infirmity is strengthened and confirmed as years advance? In the midst of all these uncertainties, we do wrong to the two classes of deaf-mutes—one congenital, the other *post nativitatem*—when we declare that the inquiries concerning consanguinity ought only to be carried on with reference to the parents of the former class.

But these objections are entirely theoretical; in practice there is one, perhaps the most serious of all—this is the difficulty of the distinction mentioned after the list of questions annexed to the files, of which the answers are exposed to numberless chances of mistakes; thus a great number of records contain no information; others contain doubtful answers; sometimes they are contradictory. All the world knows, indeed, that it is extremely difficult to ascertain the fact of deaf-muteness during the first months of life; in truth, parents have a great dislike to allow that their child is infirm from birth.

"We must count very little upon the information of parents," says Dr. Bonnafort, "on this point, for they will scarcely admit that whatever there is imperfect in the child was present at its birth; rather than allow any imperfection in the organisation, they lay the blame to the carelessness of a nurse or a servant. Then the parents, by a pardonable illusion, imagine that their child has heard and spoken: that they have seen those mechanical movements of the lips which maternal affection takes to mean *papa* or *mama*, and which the child *appears* to pronounce while trying to imitate the lips of the persons who pronounce these words for them so often."\*

One knows, besides, that in statistics, that which signifies most is the greatest number of *similar facts*: *particular* cases destroy one another, and do not modify the final result. I maintain, then, that we must examine the whole of the records to find if the inmates are present, have left, or are dead. Now M. Boudin writes, "Among 200 patients now present, we have found 95 who have been deaf mutes from birth." He ought to have said, "Among 200 records," for his examination was of these, not of the individuals themselves. I take in place of these 200, the whole of the records possessed by the institution—315, and in this number, doubtful cases being struck out, there are 124 who are deaf from birth. I state this result, to which I attach no importance, so that we can at will deduce from either one number or the other (315 or 124) the proportion of inmates of consanguineous origin.

Among the 95 cases of infirmity from birth, M. Boudin announces that he finds the following origins:—

Congenital origin, but not sufficiently established	-	8
Born of parents unknown	- - - -	20
„ „ not consanguineous	- - -	48
„ „ who are consanguineous	- -	19
		<hr/>
		95

"In only noticing the two last figures, we find 19 cases of consanguineous origin against 67 afflicted from birth, that is 28·35 per cent." Now, since the proportion of marriages between first cousins has been already fixed at 2 per cent., it follows that there are in the Paris asylum fourteen times more deaf-mutes of consanguineous origin than there ought to be, and that the danger of having deaf and dumb children in an ordinary marriage being represented by 1, it is no less than 41 in a marriage between relations. This conclusion is

\* *Traité des maladies de l'Oreille*, p. 595, 1860.

overwhelming. Happily, however, if we take the trouble to examine this line—"Born of parents who are consanguineous, 19"—this spectre of consanguinity vanishes. In fact, in the 315 reports which I have examined, I find 18 indications of consanguinity, the persons married being as follows :

First cousins	-	-	-	-	-	-	-	6
Issue of first cousins and others up to the 7th degree								11
Aunt and nephew	-	-	-	-	-	-	-	1
								<hr/>
								18

The six first cousins are : 1. Baillargeat (born in a flat and humid country : a female relation of the mother's deaf and dumb) ; 2. Berson ; 3. Lesavre (a first cousin of the father's deaf) ; 4. Maréchal ; 5. Margottin (the mother had her first child at 34 years of age) ; 6. Fouquet (the mother ill during pregnancy). Six in 315 give about 2 per cent. ; in 124, about 5 per cent. ; in 95 (M. Boudin's own number), rather more than 6 per cent. How, then, can M. Boudin explain even by his own figures the declaration that there are at the Paris asylum in every 100 inmates "16·41 who are the offspring of marriages between first cousins?" M. Boudin has evidently made a mistake between first cousins and the issue of first cousins and others, who are represented above by 11, more exactly by 7. Let us see how this number can be analysed :—

Cousins who are children of first cousins	-	-	4
„ of the 4th to the 7th degree	-	-	3
Persons <i>supposed</i> to be relations by the similarity of name	-	-	4
			<hr/>
			11

It will be granted me, I hope, that the 7 last cases are of no value ; as to the first 4, I choose them simply because marriages between the issue of first cousins are not comprised in the official statistics. We have, then, only to take account of 6 + 1 of children really born of consanguineous parents, among a total of 315 records. That will be about 2 per cent. Now, I found *at Paris*, 1·4 per cent. of marriages between first cousins, and I believe that I am right in thinking that the proportion is much greater for all France than for Paris alone. It may be equal, according to my ideas, to 3, 4, or even 5 per cent. In some places it is 8 per cent., and occasionally above this number. In taking, then, the mean of 2 or 3 per cent., I believe I am under the real number, and this mean is precisely equal to the proportion of consanguineous children in the asylum at Paris.

The difference in the result of M. Boudin's inquiries and my own arises from the difference in these inquiries: I have only admitted into the question marriages between first cousins, and the weakly issue of these marriages; I have proved the inaccuracy of the records of consanguineous marriages; I have rejected the elimination of those deaf-mutes whose infirmity has only been established after their birth; it is for you to judge if I am deceived by following other errors than those of our learned colleague; and whatever opinion M. Boudin may have of my criticism, I beg him to be indulgent on all I have been able to say touching the distinction between deaf-muteness of birth, and that which happens afterwards; for it is himself who has set me the example on this point. In fact, whilst he has endeavoured to determine the number of the deaf-mutes, who, in France, owe their origin to consanguineous marriages, he fixes its proportion at *one-fourth*, not one-fourth of the number of deaf-mutes *by birth*, but at one-fourth of the *total* number of deaf-mutes, a number he values at 36,000.\*

In order to complete this analysis, I should like to be able to speak now of M. Brochard's statistics—statistics which rely on the same data as those of M. Boudin, and which are even more summary, since they consist only of three lives. As to those attributed to MM. Landes, Piroux, and Périeu, let us see what can be said of them.

The *Impartial* contains the very interesting inquiries of M. Landes, now a professor at Paris, upon the deaf-mutes in the asylum at Bordeaux. Now, M. Landes has found 24 married relations (as distant as the fourth degree) in 287 *families*, whose children have been inmates of this asylum, from 1839 to 1859, which gives a proportion of about 8 per cent., which ought to be reduced if we take no notice of any except *first cousins*. M. Boudin, in speaking of 24 consanguineous *deaf-mutes*, has committed a double error: believing that it regarded consanguineous deaf-mutes, he placed the number opposite *deaf-mutes by birth*; now, I repeat, it had reference to 24 consanguineous *families*, not to 24 deaf-mutes. The statement attributed to M. Landes, that there were at Bordeaux 30 first cousins in 100 deaf-mutes, reposes then on a double mistake, which it would have been easy to avoid.

I had the pleasure of speaking to M. Piroux last year, on the subject of his remarkable institution at Nancy. At that time, M. Piroux had made no inquiries, and could give no information about the question I was studying. Since then, M. Piroux has published a statement which referred to 612 cases of deaf-muteness received at his

\* Dangers des unions consanguines, p. 40, 1862.



establishment from 1828 to 1863. He arrived at his conclusions by *retrospective* inquiries, comprising in them the most distant degrees of relationship, so as to obtain 15 and 17 per cent. as a maximum. Why then does M. Boudin speak of 21 per cent.? But if we take from Piroux's documents the relations beyond the third degree, we only find, in thirty-five years, forty-two who owe their origin to parents who are consanguineous, which is a little more than 6 per cent., the number at which we can in France approximately value the marriages between relations.

As to M. Perrin (Lyons), I begged Dr. Jantet, a distinguished physician of that town, to ask him about the statements mentioned in M. Devay's book, and pointed out in M. Boudin's essay. Now here is M. Perrin's answer, "I have never made any statement on the subject of deaf-muteness caused by consanguineous marriages. They were merely some verbal data which I gave to M. Devay. I had hardly remembered the fact myself. Besides, no register of this establishment indicates cases of deaf-muteness caused by marriages between cousins."

So this is what we have to say concerning M. Boudin's numerical data, in name of which some authors have dared to write that it was not any longer possible to contest seriously the dangers resulting from consanguineous unions! As if even one of the figures could be considered as important! As if even one of M. Boudin's disciples gave himself the trouble of examining into his researches! And when we consider that these tables are copied from one book to another, without explanation, with the title of an authentic document, unopposed and incontestable, we cannot be astonished at the slowness with which science progresses; and we are obliged to regret that the spirit of examination and criticism is not in our days held in more honour.

In continuation, I believe I have shown that the statistics gathered by M. Boudin concerning the consanguineous origin of deaf-muteness are entirely incorrect, because the numbers which he has announced are *all* inaccurate or wrong; that is to say, the number of marriages between first cousins, which is not 0.9 per cent., but much higher, and of which we are at present uncertain; the figures determining the number of records concerning deaf-muteness from birth in the institution; those of the deaf-mutes of consanguineous origin, and consequently the conclusions drawn by M. Boudin from this last number, with those of marriages of the same nature.

One figure alone is right. It is that which concerns the number of the deaf-and-dumb in the Paris asylum proceeding from marriages be-

tween nephew and aunt; and this figure is 1. Whence could M. Boudin have discovered that the danger of producing deaf-and-dumb children from this description of marriage was equal to 70 per cent.?

#### IV. INDIVIDUAL OBSERVATIONS: ENGLISH AND AMERICAN DOCUMENTS.

I have now to examine the numerous pathological facts which have been placed to the account of marriages between cousins. For these marriages do not only produce deaf-muteness, but also blindness, colours on the retina, "albinism", epilepsy, idiocy, mental aberration, sterility, scrofula, abortion, numberless deformities, children with six fingers (*sex-digitisme*), hare-lip, "all the predispositions", and even "red hair and freckles on the skin"! Most of these facts rest on one or more individual observations, more or less convincing, and even more confusing; so, in order to analyse in a methodical manner the question on which we are employed, it is convenient to divide the documents we are about to examine into two groups.

A. Facts which relate to observations concerning sickly children in whom is discovered a consanguineous origin—the *indirect method*.

B. Facts which relate to direct and individual observations concerning married relations—the *direct method*.

A. *The indirect method.* When we have satisfied ourselves that a patient is suffering from a disease or infirmity the origin of which is not recognised, we are often led to examine into the truth of some hypothesis concerning all the individuals who show the same peculiarities as the first one. Now, we cannot fail to arrive at an affirmative solution, whatever may otherwise be the supposed cause, if the observations are made on a great number of cases; for we are almost sure to meet with the hypothesis a certain number of times, and by this we are led to give a particular action, great or small, to the hypothesis, in the production of this disease. To explain my idea better, I will suppose that a pathologist has, in his own mind, attributed congenital blindness in a great measure to syphilis, and, in order to verify his theory, he asks the parents of the blind, who are under his care, if they have ever had syphilis. Now, I will suppose that in one hundred cases he is answered twenty-five times in the affirmative; he hastens to declare, with the usual reservations, that in one-fourth of the cases of congenital blindness, syphilis has appeared to be the true cause of the infirmity.

Allow me to proceed farther, and to suppose that we have to do

with a very honest, but obstinate theorist, impelled by the very laudable desire of recommending future parents to avoid syphilis. What will this pathologist do? he will forget the number of observed cases; he will publish his twenty-five observations of congenital blindness due to syphilis; his publication will provoke a hundred other communications of the same kind, and at the end of a certain time there will be a certain budget of facts collected which will prove, so far as evidence goes, that the most positive cause of blindness is syphilis in the parents.

Here, again, is another pathologist, who considers that the abuse of strong drinks is the principal cause of blindness; a third, who attributes it to the illness of the mother during pregnancy; a fourth, to the habit of sitting up late at night, etc. Each of these causes will verify itself a certain number of times among the parents of a hundred patients, and very soon it will not be the causes which are wanting for ailments, it will be the ailments which are wanting for the causes!

This method, which has a prodigious effect upon persons who are unaccustomed to scientific argument, has been employed with success in the question of consanguineous marriages. Whenever anybody saw a deaf-mute, he endeavoured to find out about the relationship of the parents, and these researches were sent to the Academy of Science, or to our Society, and published in different collections of reports. Such are most of the facts recorded by MM. Devay, Forestier, Duteval, de Ranse, Trousseau, Ponsin, Rizet, Balley, Broclard, Chazarain, Chipault, etc. I take sixty to eighty as the maximum number of cases determined by ten or twelve physicians.

There is much to say about these facts alone; and in my former work, I showed that, in the great majority of cases, they had neglected to mention circumstances which would have rendered their observations useful, not only to the theory which is being considered, but for etiology in general. The small number of cases observed, according to scientific rules, evidently admits of no conclusion. I will allow, for a moment, that a hundred cases have been collected in which children tinged with disease have been born of consanguineous parents in good health. In these hundred cases, they reason thus: nothing explains the illness of the child excepting one circumstance alone, the consanguinity of the parents; therefore, consanguineous parents bear weakly children. What can be more absurd? what more specious, or more "showy" for the generality of people? Take a certain number of deformed children and choose from them one hundred whose fathers, otherwise well formed, have brown hair or any other

distinctive mark. You say, at the same time, that you have no reason to give for these deformities (this is the rule), and add, one particular feature is common to all these patients: their fathers have brown hair; hence men with brown hair have deformed children! Your etiology will be as well founded as the foregoing.

I shall be told that there is no connection between brown hair and consanguinity. In fact, one cannot see by what reasoning brown hair can lead us to gibbosity or club-feet. So be it; but who would not be curious to ascertain by what path consanguinity can, *ipso facto*, lead to *sexdigitisme*, or deaf-muteness? For we must insist upon this point, they do not accuse inheritance combined by the union of two diseased individuals, of near relationship, and bordering on some pathological result; which would be a theory worth examining, because it refers to an important part of physiology—inheritance; no, they desire that consanguinity should produce these terrible effects in the absence of all inheritance, in spite of inheritance! They wish to introduce a new law into science. We have seen with what trouble, and at the end of how many years, the French physicians have been able to gather a few cases of infirmity among children of consanguineous parents. It has not been the same, however, in America. There Mr. Morris has observed 4013 children of consanguineous birth, among whom he has found 2580 cases of deformity.

It is prodigious: Mr. Morris must have passed his life running after consanguineous children, to have been able to “study” 4013 cases! M. Boudin mentions this. It is true, he does not mention his authority,—a fact which takes away from the statement, not, indeed, its miraculous character, but all its value. Nobody has, besides, been able to inform me of Mr. Morris’s scientific position. M. Devay has quoted statistics even more extraordinary than these. “In Ohio,” he says, “among 3,900 children (consanguineous), 2,490 are afflicted with serious deformities or complete idiocy.” The authority for this document is not any more indicated than that of the foregoing one in M. Boudin’s work: three of our colleagues have endeavoured to discover it, but without avail. I have denounced this extract as unworthy of credit, and nobody has relieved it of this imputation. But I render this justice to M. Boudin, that, in the new edition of his *Essay*, he has not reproduced it. The same fate is doubtless reserved for the *magnificent* statement made by Mr. Morris, who, besides this, says absolutely nothing. I refer on this point to what I stated above.

B. *The direct method.* I give this name to the method which consists in observing *directly* consanguineous parents and their offspring.

It is certainly superior to the former. It would be excellent if it were possible to examine *every* consanguineous marriage, in one or many departments, and to calculate the number of infirm children born of these marriages. We could compare the proportion thus obtained with that which would result from the examination of all other marriages; if the possible excess from one side or the other could not be connected with any other cause, and if a like result was obtained in one or many departments, the question would be settled. Nevertheless, it appears doubtful to me that such a work would be possible; whilst the plan proposed above, which is quite as demonstrative, is certainly quite easy of execution. But the statistics which have been published are far from offering this guarantee; they seem to us to be tainted with a radical fault. It seems, at first sight, that if we could compare a hundred marriages between first cousins with a hundred ordinary marriages, the examination of the respective registers of births, still-births, invalids, and deformed children, would bring us to some result; and yet the least reflection will show the fallacy of this; for one can at will choose a hundred ordinary marriages whose issue may all be very healthy, or a hundred other marriages whose issue may be more or less sickly: the same with reference to consanguineous marriages. One can guarantee more than a hundred marriages without one case of a weakly child,—a method which makes such a path for the progress of a theory is thoroughly wrong. This is why the statements of M. Bémiss (34 marriages, and 192 children, of whom 47 are sickly) and of Mr. Howe (17 marriages, and 95 children, of whom 44 are idiots and 12 scrofulous) are of no value whatsoever. What would be said if I went to a child's hospital, and taking the names of the parents of the first hundred cases of deformity I may happen to meet with, should thence conclude that there are a hundred cases of deformity to *every hundred marriages*? However, in France the adversaries of consanguineous marriages have not been so happy as M. Bémiss and Mr. Howe. M. Devay has observed a hundred and twenty-one consanguineous marriages; although he does not mention the total number of children, let us consider them, according to the mean (3·5 children to each marriage), at 423. Now, M. Devay has only found thirty-five cases affected either by true pathological disorders, or by accidents which have destroyed the powers of generation. Among these 423 children born of consanguineous marriages, we should expect to meet with deaf-mutes, blind, idiots, etc., etc. Nothing of the kind. We find 22 cases of *sterility*, with or without abortion, 5 cases of club-foot, 1 case of anencephaly,

2 of hare-lip, 1 case of *ichthyosis*, 1 of *enchondroma*, 1 of *spina bifida*, 2 of albinism. We are thus a long way from 66 idiots and scrofulous patients among 95 children !

In continuation, M. Devay finds 35 cases of disease, or pathological accidents, in 121 marriages, and 423 children. I believe that this proportion is equal or inferior to that which we should discover in 423 children *not* born of consanguineous parents.

Among the authors who have chosen this direct method, some have *proved too much* ; the others have not proved enough. They have established nothing definite against consanguineous marriages. Such is the balance-sheet of the numerical documents on the question now before us.

#### V. CONCERNING THE ARISTOCRATIC TYPE AND PURE BLOOD.

M. N. Périer, in his second paper ("Essai sur les Croisements Ethniques"), has treated the subject of the old noble class of society with too great skill for me to say much on the subject. He has shown, that, so far from offering disadvantageous conditions of health or longevity, the aristocratic classes are even exceptionally favoured : and when M. Devay quotes, as supporting his theory, the fact that out of 61 persons belonging to the most illustrious families of Dauphiné, who composed the company of Bayard's men (*hommes de Bayard*), there remain only 5 descendants, he gives to these families a descent more lasting than is the average. Benoiston de Châteauneuf has besides established that when noble families degenerate, it is *because* they degrade themselves by marriage. If Dr. Nott (quoted by M. Boudin) pretends that the reputation of the House of Lords in England would have been extinguished long ago, "*unless the crown had continually created new lords from among the robust sons of the people*," Dr. Nott and those who quote him completely misunderstand the conditions under which an English citizen is raised to the peerage. We can affirm that, for a very long time, account has been taken of only two circumstances as deserving this dignity—long and useful services and personal fortune. Do we ordinarily designate by the name of "*robust sons of the people*" the magistrates, the generals, the learned men, the rich landowners, who fulfil the conditions required for a peerage ? Dr. Nott's remark is then quite incorrect.

Besides, one must be endowed with very little of the spirit of observation, not to perceive the great part which in our days is played in various lands by the descendants of our ancient aristocratic families : in literature, in politics, in parliamentary life, in the navy, in the

army, even in science, and this in spite of the hostility of the age, the destruction of fortunes and privileges, in spite of alliances with citizens. We can say, without hesitation, that wherever we find dominant and privileged classes who know how to maintain their exclusiveness, they know also how to maintain their dominion. To give a lively example of it, how can one suppose that in those colonies where the negroes are governed by white men, these latter could maintain their power if they intermarried with the blacks? I hope, besides, to bring forward some decisive facts concerning families of consanguineous origin in ancient Greece. I limit myself here to the declaration, that having consulted many Englishmen concerning the personal valour, appearance, and beauty of members of the English aristocracy, they have declared that they are distinguished from the rest of the nation not less for their physical valour than for their intellectual strength.

But the question takes even larger proportions than these. We have passed from individuals to families, from these to classes, from classes to races, and this extension given to our discussion is not only logical, it is necessary; for, to say the truth, the only doctrine we now sustain is the superiority of pure races over crossed breeds.

The superiority of individuals of consanguineous origin is but a mediate, remote corollary, to the superiority of pure races. And since a fact studied in the case of an individual is surrounded with elements much more complex than it is in the case of a group, it follows that we can affirm concerning a collection of individuals a law which, as regards the individual himself, is somewhat rigorous. Exceptions, in fact, disappear and are confused in the collection of general facts; but, if we neglect these, we risk the deduction of false rules, established by an insufficient number of particular facts. This is why I do not accept the reproach of *confusion* with which M. Boudin charges me in the last edition of his *Mémoire*. I have endeavoured to prove that ethnic cross-breeding, so far from favouring its issue, engenders mixed breeds inferior in proportion to the physiological differences between the crossed races. "M. Dally," says M. Boudin, "confounds here two things destitute of all joint connexion,—cross-breeding in families, and cross-breeding in races." On this point I appeal to the opinion of my colleagues: far from confounding these two points, I render one subordinate to the other.

A pure race is an exalted result of primitive consanguinity. Two couples who unite indefinitely their branches, without marrying out of their circle, ought to constitute a race. The alliance remains con-

sanguineous so long as no fresh element establishes a cross; it is when we apply this latter term indifferently to races and families that we cause confusion. The alliance of two families of the same stock is a consanguineous alliance, whatever may be the distance of the relationship; when these stocks are as distinct as are, for example, the Negroes and the native Americans, one has an instance of a *mixed breed*; we have an instance of hybridity when the anatomo-physiological differences are so considerable as to make us consider the two generations as different species; and further, when the issue is not fruitful between them from the first generation. Such is the probable, but not sufficiently established case, of the Anglo-Saxons on one side, and the Australians and Tasmanians\* on the other.

I willingly admit, however, that the definition of the terms *consanguineous*, *cross-breeding*, *mixed-breeding*, *hybridity*, leave much to desire, and that the sense of their meaning is far from being established; I admit, besides, that the multiplication of European cross-breeding renders necessary a special designation, in order to distinguish the alliances of relationship from consanguineous alliances, properly so called; and in the absence of any special term, I admit that we can without inconvenience, restrict the signification of this latter term so as to call *consanguineous* marriages, marriages of relationship. But with regard to this concession, we must be prohibited from calling every marriage, which is not consanguineous, a *cross-marriage*; for a cross-marriage is understood of two individuals of distinct races. Clearness of expression, this condition of all scientific harmony, is completely wanting, it may be seen, in our honourable opponents.

These explanations having been given, I return to the subject of pure races. We have seen that M. Boudin denied any connexion between the question of races and that of families. "As to the danger there is in a brother marrying a sister, or a father his daughter, it does not follow at all that there ought to be any benefit when an Englishman marries a Tasmanian, or a Frenchman a Hottentot." This inference is very amusing; we give our opponents credit for its invention, which cannot fail to cover them with ridicule. I shall not care to profit by the liberty which such a thing offers me; and I shall not forget, I hope, the respect which I owe to a colleague who has given so many proofs of his love for science. But am I wrong in thinking that M. Boudin, even in the title of his essay, admits the connection of the question of races with that of families?

\* See on this point Dr. Nott's *Types of Mankind*, p. 572; and M. Broca, in the *Journal de Physiologie*, p. 654, 1860.



In the first edition of his paper I read, "necessity of cross-breeding in the human race;" in the second, "necessity of the cross-breeding of families." If cross-breeding in families is necessary, the same thing among races is also necessary; and if "crossed" families are superior to pure blood, why refuse the same superiority to crossed races? Is a race anything more than a large family? Not to suppose that a creation of millions of human beings was produced at once, has not a race its origin in a family, which had for its own origin two persons? And till the day when this family is *crossed*, who can say if the theories of MM. Devay and Boudin are correct? who can say what it has engendered, deaf-muteness, blindness, cretinism, idiocy, hare-lip, etc.? Cross-breeding has happily come to remedy all this, and from this day the superiority of the new race is dated,—this is what our opponents must admit, if they wish to be logical. However, those who believe in the unity of origin of the human race, ought to find themselves in some degree embarrassed in establishing the first *cross*; and those who bring forward sterility among the inconveniences of consanguineous marriages, ought to be astonished at the prodigious multiplication of mankind.

Besides, it is quite recently that our eminent fellow-member thought proper to separate the cause of pure races from that of pure families; his predecessors and his disciples accept all this connection—quite fatal to his own theory. M. Devay, in the answer which he did me the honour to dedicate to me, has said nothing which could make one think that he rejected it. But he has left without refutation the arguments which I have drawn from the inferiority of crossed, half-bred, or hybrid races. M. Boudin, who had no motive in answering on this point, since the two questions have no connection in his eyes, has, however, taken the pains to oppose me with several extracts from the writings of Hombron, D'Orbigny, Lallemand, and Levaillant. Relying myself upon the authority of Jacquinot, Nott, Van Amringe, H. Smith, and Broca, I have urged that the half-breed between blacks and whites were probably sterile between them, and in the absence of all cross-breeding in return, beyond the third or fourth generation. M. Boudin answers that, according to M. Hombron, "marriages between mulattoes and mulatto women are extremely fruitful." Now the word mulatto is understood to be the *first* half-bred generation, and not the third or fourth. The extract from M. Hombron has, therefore, nothing to do with the question. The same may be said of the extract taken from Levaillant, who asserts the same regarding the Hottentots and the whites.

There remain D'Orbigny and Lallemand. The first speaks of cross-breeding between the "various American races" (*id est*, of the same race), which he considers superior to the two types *mixed*. It is a simple *mélange*. Lallemand quotes the families formed by Germans married to women of the South of France, as being very beautiful and healthy. This is the opinion of Lallemand; it is important, but it must be noticed that he speaks of a cross between two very similar races. And while we are speaking of this author, I hope M. Boudin and his copiers will allow me, for the sake of the learning of their successors, to restore an important part of a sentence, the absence of which is not even indicated in the abstract which he makes from it. Lallemand said: "When alliances are too much restricted, *they tend to redevelop the predominance of their distinctive characters*, and finish by breaking the equilibrium necessary for the normal development of the constitution." M. Boudin has omitted the italicised words—words very important, too, since they show the reasons for which this learned man disliked consanguineous marriages: the reason of excessive or morbid inheritance. Now one knows that, very far from admitting that it is in virtue of the laws of inheritance that consanguinity is dangerous, MM. Devay, Boudin, and others wish that it should be *ipso facto*, in the absence of all morbid inheritance, and *in spite of inheritance*, that it should produce its terrible consequences. I hope that I have restored to M. Lallemand's extract its true signification.

I return now to pure races. If M. Boudin rejects the connection of the question of races with that of families, of what value then are the observations he has made on the Jews for his actual theory? Why these statistics and extracts? Recognise the joint connection, or suppress these superfluous documents. But I understand it is not so much that the Jews, as a pure race, may be subject to collective infirmities, but because marriages between very near relations are more frequent with them than with Christians.

Such is not the opinion of the chief rabbi at Paris. This ecclesiastic remarks that, "between cousins, alliances are everywhere permitted with slight hindrances from canon law, which are made to disappear without any difficulty." All the world is of the opinion of the rabbi. All the world knows that, since certain great historical events which have not contributed to the prosperity of the French nation, the Roman Church has never refused to grant the necessary canonical dispensations. M. Boudin, however, appears to contest it. "It is merely a personal opinion of the chief rabbi, and one which

seems very questionable." Very well; let M. Boudin question it! It is the case of applying the numerical test. It is, then, a pure supposition of M. Boudin, in considering that consanguineous marriages are more frequent among Jews than among Christians; and, lest our colleague should come at last to accept the connection which he rejects, he is no longer authorised to use facts for his cause which he can lay to the account of the Jewish race. These facts belong to the question of the value of pure races, and not to that of relationship marriages.

Now what are these facts? It is asserted that "the Jews have lost the strength and beauty of their race." "Nobody," says M. Devay, "denies this fact." This is incorrect, for the fact *is* denied: if it had been proved, it would besides be susceptible of other explanations. It might be said that it is precisely since the barriers have been lowered which separated the Jews from European Christians, that this decay has been discovered; and this explanation would be quite favourable to the question of pure races. And more, we may remember that this singular development, under all latitudes, of this race, so cruelly persecuted both by Romans and Christians, has been effected by means of a forced consanguinity: now, since the decay of a race is a very common historical phenomenon, the causes of which are extremely complex, complete ignorance is shewn in attributing it to a cause which is not found among other degenerated races. In every case, the Jewish race, standing alone in the midst of European movements, has shewn an unequalled vitality. This example, then, is unhappily selected; it is among those which the partisans of consanguineous races like to quote, in relying on the inquiries of M. Boudin himself. But this decay is so problematical, that it is, I believe, useless, to insist on it. That which is less doubtful, is the fact that the Jews furnish, in Germany, a larger proportion of lunatics than are found among the Christians. In fact, it seems from tables produced by M. Legoyt, that there is one lunatic among 908 Romanists, 967 Protestants, or 514 Jews (Bavaria). Analogous figures are produced for Hanover, Silesia, and Wurtemberg. These tables are of German origin: M. Legoyt, so far from wishing to prove nothing from this, adds, "Must we see in this frequency of lunacy among the Jews an influence of race, or merely the consequence of this fact—that *they inhabit those towns, and exercise those professions, the most exposed to economic crises?* Must we see there, like Dr. Martini, the influence of the fact that marriages of near relationship are more numerous among the Jews than among

Christians? Ought we, perhaps, to admit the concurrence of these three causes in the production of this phenomenon?"

If the German tables are correct, it is certain that the above-named cause is much more probable than the others. We know, indeed, that towns furnish a larger contingent of lunatics than the country; and we know, also, that the professions generally followed by Jews are those which, beyond all proportion, predispose in the highest degree to mental aberration. Such are the artistic, financial, and learned professions. Unhappily, we do not possess in France any document which would allow us to check the German statistics. We do not even know exactly the number of the Jewish population of France.

Nevertheless, a Berlin physician, M. Liebreich, has drawn up at the deaf-mute asylum of that town an overwhelming account, not with regard to consanguineous marriages, which are not more frequent among Jews than Christians, but with reference to the Jewish race. Among 223 deaf-mutes, born at Berlin, M. Liebreich has found 23 Jews; and in the total population of 341 deaf-mutes, 42 Jews. Whence it follows that, whilst the Jewish population of Prussia only represents, according to M. Boudin, the sixty-second part of the total population, the deaf-and-dumb Jews constitute about the eighth part of the total number of the deaf-and-dumb in the asylum at Berlin.\*

Far be it from us to doubt the scientific honesty of M. Liebreich; but when foreign statistics reveal to us a fact so strange and so contradictory to that which we can prove in France, we can only accept his statements with a great deal of reserve. We have proceeded in the same manner with the statistics offered by M. Boudin, whatever opinion we may have of his good faith; and it will be seen that we have done right.

The chief rabbi has told us that the Jewish population of Paris has

\* In M. Boudin's first edition, the words "223 deaf-mutes born in Berlin", did not appear in the text. It was merely a question of the total population, 341. I have, then, rightly objected that the inmates of the Berlin asylum consisted in part of Jews coming from all parts of Germany, and that it was not right to take the number 42 in order to compare it with the Jewish population of *Berlin*. M. Boudin (and after him M. Chipault, who, without recognising my work, has contented himself with copying M. Boudin) asks me if I admit that a person can be born at the same time at Berlin and in some other place? I will content myself with answering MM. Boudin and Chipault that I am not one ready to see a double meaning in an expression. In truth, there *is* at the foot of p. 14 a note, which corresponds to a false reference. If M. Boudin refers to this note, he ought to have said so. I spoke of the text, and I maintain that in this, it was *not* a question about the deaf-mutes born in Berlin.

raised itself to more than 25,000 souls: in this number there are not *four* deaf-mutes. The asylum at Paris, containing more than 200 of the same class, has only two Jews among them, or 1 per cent., whilst at Berlin there are nearly 13 per cent.; there would be at Berlin 27 deaf-mutes in 10,000 Jews, whilst at Paris, in the same number of Jews, there are only two or three deaf-mutes. One statement is here certainly in opposition to the other.

M. Boudin, in his answer to the chief rabbi, gives us a particular specimen of his way of understanding statistics. We have seen that the Paris asylum contains only two Jews, both coming from Bordeaux. Now, our ingenious colleague, admitting with the chief rabbi that there are in France 100,000 Jews, or one Jew among each 360 Christians (M. Boudin says one Jew among 360 Frenchmen, as if the Jews were not French), concludes that only one deaf-and-dumb Jew ought to exist among 360 deaf-and-dumb Christians. Unfortunately, there are only about 200 deaf-mutes in the Paris asylum. One ought only, then, to find there one "demi-Juif" (*sic*); now there are *two* of them! "The consequence is," says M. Boudin, "that the *real* Jewish contingent of deaf-mutes in the Paris asylum exceeds by four times the *legal* contingent."

M. Boudin is very particular: not always so in his calculations (for it is not 0·5 of a Jew he ought to find, if the Jewish contingent were *legal*, but  $\frac{1}{20}$  of a Jew—0·55), but in his appreciations. What are statistics worth when thus understood? If M. Boudin had only found a quarter of a Jew, would he have said that the real contingent was inferior by one-half to the *legal* contingent? and if the observation had been made before these two patients came from the Bordeaux asylum, must we conclude that the Jewish population furnishes four times less deaf-mutes than the Christian population? Besides, M. Boudin is not quite "up" on the subject of the Jews of the Paris institution. In the first place, they were not deaf-mutes from birth; one became so at two years of age, the other at four years. Now, M. Boudin strikes these cases out of his statistics, and reproaches me for not admitting the justice of doing so. Then why speak of the two Jews in question, who have become deaf-and-dumb long after their birth? In the second place, the parents of these two Jews (there are three of them now) have *no* tie of consanguinity between them. So that, in truth, there is not in the Paris asylum even the least fraction of a Jew of whom M. Boudin can take notice in his statements.

I regret to be obliged to enter into such details; but we can only

obtain a fair idea of the value of such statements by examining them minutely. And, to finish with the subject of French Jews, I may remark that, if there are in Paris four deaf-mutes in 25,000 Jews, the proportion is less than that of the entire population of the department of the Seine, where, according to the official reports, there is one deaf-mute among 4,694 inhabitants; equal to about five in the 25,000. M. Vaisse, chief of instruction in the asylum at Paris, has done me the honour to write to me, and give me important information; and he adds that "the Paris institution has only brought up a very inconsiderable number of Jews. *It has sometimes contained one or two, and often none at all.*"

We see, then, that it would be advantageous to give more authority to M. Liebreich's statement by examining it carefully. Now, if it is verified, we shall have a curious fact before our notice, and probably, a fact concerning *a race*, not with reference to marriages between cousins, which, we repeat, cannot be more frequent in Prussia, where the Jewish population is numerous, than among the Christian population. But why has not M. Liebreich inquired into the relationship of the parents of the 42 deaf-and-dumb Jews at Berlin? This forgetfulness astonishes us on the part of an adversary to consanguineous marriages. If, in fact, a large proportion of the parents of these 42 Jews were blood relations, we should certainly have been informed of the fact. Perhaps, as at Paris, they have not been able to prove a single case of consanguinity. At all events, we have the right to presume that it is so.

But who speaks of the degeneracy of the Jews? Who produces foreign papers which seem prejudicial to the healthy condition of the race? It is the author of those ingenious works on the "non-cosmopolitism" of man; he who endeavours to show that the Jews can perpetuate themselves in all latitudes, that their population has doubled in fifty years, that the mortality among their children of tender years is less than among others by more than one-third in some countries, by one-half in others; who, in fact, would make us conceive the most unbounded hopes for the Jewish race.

We have already said too much about the Jews to permit us to broach the subject of the value of other races, more or less pure; a question which sooner or later must be discussed; for there is not one which is grander or more worthy of our anthropological studies. In the meantime, I commend to the perusal of my opponents the following extract from M. Gobineau's work—a work of great importance, too—*Essai sur l'inégalité des races humaines*. "I think, then, that

the word *degenerated* as applying to a people, ought to signify, and does signify, that this people has no longer the intrinsic value it formerly possessed, since there is no longer the same blood in its veins, and the successive "alloys" have gradually altered its value. In other words, with the same *name*, it has not preserved the *same race* as its founders; so that a man of the decayed period, what we call a *degenerated* man, is a different person, in an ethnic light, from the heroes of the golden days of the race. The heterogeneous elements which henceforth predominate in him make up an entirely new nationality, and one unhappy in its originality: he now only belongs to those he called his ancestors, in a collateral line. He will in the end die out, and his civilisation with him, in the day when the primordial ethnic element is found to be so subdivided and overwhelmed by the mixture with strange races, that the virtuality of the element will not henceforth be able to act with sufficient power."\*

#### VI. FACTS FAVOURABLE TO HEALTHY CONSANGUINITY.

I have hitherto restricted myself to the criticism of the facts which have been brought forward as unfavourable to consanguineous marriages. I shall now have a good many observations to offer which are sometimes negative in regard to the asserted dangers, and sometimes very favourable to these unions. I have, in fact, gathered together more than thirty cases of this order, comprising about 100 healthy children: these cases have been pointed out to me either by physicians, or by persons who know nothing about medicine, to whom, in general, I have not communicated the design of my researches. But what can these facts signify? Will they escape the criticism which I have myself made on the facts stated by my opponents? Assuredly not. I could collect a thousand of them; but that would not help me. What we want are *comparative* reports, and that is why M. Boudin's method, *when properly applied*, is an excellent one.

I have also collected some pathological cases among consanguineous children. Three of these cases come from a friend who acknowledged himself that there is still much to describe concerning hereditary maladies, but who has not been able to give himself up to these researches. One of our colleagues, Dr. Defert, has been more fortunate; he has been able to observe two cases, one in his own family, the other among his patients. The first case has reference to a person who marries a woman with whom he has no tie of relationship. He has two children, who both die. Having become a widower, he mar-

\* Journal of the Statistical Society, Paris, 1853, vol. i, p. 9.

ries his first cousin, and as issue of this marriage, he has one healthy girl, who is now of a "certain age." In a second case of marriage between first cousins, the young wife had three miscarriages, one after the other, each at six weeks. We should be inclined to put this case to the charge of consanguinity, if we were ignorant of the fact that the young woman had been constantly liable to dysmenorrhœa since puberty, and of this character too, that she had at every return of the menses a considerable uterine congestion, and finally uterine hemorrhage. We see how difficult such a study is!

It is not my intention to speak of the thirty cases which I have gathered together; if I have quoted these two, it is because they were particularly observed. But I cannot, however, omit the fact that I have for a long time been able to watch a marriage between first cousins in a Russian family; one of the persons is afflicted with a scrofulous affection which has caused some very severe disorders; his wife, although naturally of a good constitution, is far from being well, but from entirely accidental causes. They have had five children: only one has been delicate in its infancy: at present they enjoy the best possible health, and they have passed the most critical periods of their first youth without any accident. They have, in fact, much better health than their parents. We notice, besides, a considerable number of consanguineous marriages in this family: all with excellent results.

The documents communicated to the Academy by Messrs. Bourgeois and Sequin are more conclusive than the preceding facts. We know that M. Bourgeois has brought forward the history of his own family, in which 68 marriages, all of them "overburdened with consanguinity", have produced excellent results. M. Sequin has supported the conclusions of M. Bourgeois before the Academy of Science, and he has given the history of ten consanguineous unions between his family and that of Montgolfier, without one case of a weakly or deformed child.

Our colleague, M. Lagneau, whose scientific mind we all know how to appreciate, has quoted to us the example of the families P—— and N——, "whose members, after having been united eight times among one another in the space of eighty-seven years, from 1694 to 1781, have still at the present day healthy descendants in this country."

Again, a distinguished pupil of the Paris hospitals, M. B——, has communicated to me an analogous case drawn from his own family. I here give a copy of his note on the subject:—

"It seems, from information which has been handed down to me



by my family, relating to a period of about one hundred and fifty years (*i.e.*, counting from the great grandfather of my father), that five generations have married among their first cousins: the degree of relationship has never descended beyond the *first cousins*, excepting in *two cases*, where the *daughters of first cousins have been married by their second cousins*. These five generations have contracted a certain number of marriages which I am not able to particularise, and in which the mean number of children has been 3 or 4. The total number of branches as direct as collaterals has been 120 to 140. There has been no idiot or deaf-mute met with. Two females only have died of consumption; one without any appreciable cause, the other from catching cold. One only has been seized with senile insanity at the age of 68, three years before her death. No predisposition, except the rheumatic predisposition common only to a few individuals. My family has included many physicians, who, although imbued with prejudices against consanguinity, have themselves judged of it by its results, and have only been able to encourage it. I may add that the number of "branches" is the more surprising since a great number of them have devoted themselves to a life of celibacy, or have made religious professions."

I have particularly copied this note of M. B—, and have given it literally. He did not wish his name published, and is going himself to marry his first cousin.

These three facts are, besides, the more remarkable; for if consanguineous marriages presented any dangers whatever, it should certainly be in these cases of consanguineous unions, multiplied between two families. Well established facts of this description are worth a hundred contrary facts; for we must not forget that we only think of collecting observations in which there is something unusual, and accordingly, the ailments of consanguineous children are very quickly quoted and requoted, while consanguineous marriages, which have nothing extraordinary about them, are forgotten. One more important remark for the sake of criticism, and it is, that isolated observations concerning the diseases of consanguineous relations prove absolutely nothing, for these diseases are not special to these individuals, it is only their *relative* proportion which can prove anything. On the contrary, isolated observations on consanguineous marriages are inconvenient, since their offspring prove that the dangers are illusory. This difference, which seems a paradox, may be illustrated by the following example. If I place two first cousins in an island, and at the end of several generations the island is found peopled without having one case of birth infirmity, I have the right to declare that in this particular case the

consanguinity of the parents has had no bad effect. But if I saw deaf-mutes appear in this same island, I have then no right to accuse consanguinity of this; for, in the first case, the demonstration of my conclusion is complete, *ipso facto*. In the second case, there is no possible demonstration; in fact, other reasons than consanguinity may have caused the appearance of deaf-muteness; and I can choose one of them according to my own fancy—according to my own particular ideas.

This example leads me to consider a new description of proofs, invoked one after the other by the opponents and defenders of consanguineous marriages. I wish to speak of certain groups of individuals, who, by reason of various circumstances, such as geographical position, traditions, professional, social, or religious differences—are isolated, and almost forced to contract marriages of near relationship. In certain small countries the aristocracy are of this class; but we will not return to this subject—we gave it our former examination. There is room for making, on the subject now before us, some researches among the islands on the French coast, at the same time being careful to guard against any local influences which may have a hand in the production of disease. We speak, be it well understood, of proportional and comparative researches, for isolated facts are of no importance. In the Oceanic islands, a number of tribes, who certainly do not shine by the beauty of the forms, are quoted from the notes of Hombron, Duroch, and Lesson, and these quotations having been made, it is pretended to have proved that in *every place* where alliances are circumscribed, consanguinity causes great ravages! The author of these quotations has not even taken the trouble to compare the congeneric tribes; he does not say a word about the form of marriage, and yet he pretends to have *proved* something!

For want of neighbouring islands, let us remember that M. Périer has mentioned, according to M. Yvan, the beauty of the inhabitants of the island of Réunion, who descend from a few couples only, and yet have known how to preserve their purity of blood,—for most of the French colonies, when they are prosperous, offer the same character; in fact, we may remark even in France itself, isolated spots or isolated groups of individuals in the heart of a mixed population; there are very few travellers who have not noticed it, and this has never been with a view of establishing their degeneracy. Among this number are most of the little fishing villages on the coast of France, where the sailor population lives side by side with the agriculturists without ever marrying among them. Such is Pauillac

(Gironde), about which my friend, Doctor Ferrier, has written me a letter, from which I take this extract:—"Pauillac contains 1,700 inhabitants, most of them are robust, vigorous, and well-made sailors; the women are renowned for their beauty and the clearness of their complexion. There is, perhaps, no other place in France where consanguineous marriages are more frequent, and where the case of military exemption is more rare." Such, also, is Granville, where the maritime population, quite distinct and isolated, are a very fine set of men; Arromanches, a little village of less than 100 fishermen, who have very little sympathy for "earth-workers;" such is, above all, Portel, near Boulogne, a village containing some hundreds of inhabitants, who are all allied among one another in the closest relationship, and who never marry among those whom they call "the shepherds" (*bergers*). Batz, in Brittany, is a commune containing 3,000 souls, about whom Dr. Voisin furnishes the following information:—"The nucleus of the population is composed of nine families. For a very long time the inhabitants of the town have married amongst one another, except in very rare instances. In that part of the country it is a title of nobility to belong to the town of Batz, and it is rare to hear of unions with the people of Croisic or Pouliguen. The inhabitants of Batz are either workers in salt-pits or fens, and pass their lives in the open air, near the sea, in the salt-marshes; their chief labour is the preparation of the salt; both men and women are extremely robust, of a good height, and perfect health. Their hygienic condition is admirable, and misery is unknown in the country. I find, besides, from my notes, that there are very few of the inhabitants who are relatives beyond the sixth degree, for the most part their relationship is of the third or fifth degree: the children are numerous, and average from two to eight for each marriage."

M. Gubler, in a recent journey, has been able to establish the extraordinary beauty of the inhabitants of Gaust, in the valley of Assau, in the midst of the Pyrénées. The custom of marrying relations is so inveterate among them that, before marrying an inhabitant of another commune, the young men of Gaust ask permission from the chief men of the place; and yet this little place contains barely two hundred inhabitants. Analogous facts are cited by M. Perier concerning the village of Uchizi, not far from Mâcon, and of the canton of Saint-Martin d'Auxigny, near Bourges. Our friend, M. Maximin Legrand has mentioned the same facts about the town of Ecuelles, near Verdun-sur-Saône; and I think that I could quote a hundred, perhaps a thousand, places in France which fulfil the same conditions.

In all this there is a special work to be done, which may demand many years of patience, which the Anthropological Society will be able to accomplish little by little, thanks to the excellent table of questions on the ethnology of France which has been prepared by our friend, M. Lagneau.

#### VII. HISTORICAL AND LEGISLATIVE DOCUMENTS.

We have more than once wandered from precise and contemporaneous facts: in order to pursue our work in historical order, we must extend the design of our researches, we must retrace the course of time, and, following our opponents in their superficial investigations, resume the analysis of legislative facts, or the traditional customs of a great many nations. But we know not where such a study, entered on conscientiously, may lead us; and the slightest examination of the documents produced in this system of inquiry by MM. Devay and Boudin, is sufficient to take from them the singularly decisive character which their authors wish them to assume. We may be allowed to make a few critical remarks on this subject.

We shall at once remark an error of judgment among our opponents. Thus M. Devay, like M. Trolong, makes much of the universal dislike of nations (and animals) for consanguineous marriages; confounding in one category the necessities of domestic morals which prohibit incest, and the metaphysical doctrines which interdict unions between cousins, our author cites the Hurons, the Iroquois,\* and the Samoyedes, among the nations who have reprobated and forbidden consanguineous marriages, and he relies on the universality of this opinion in order to prove its value. I do not insist on this confusion, common to my opponents, and which seems to consider *incest* and *marriage between cousins* as the same thing. But I note that M. Boudin is of quite another opinion; so far from considering the prohibition of consanguineous unions as an universal fact of *natural right*, he tries to establish "that among a great many nations of antiquity we find it *quite natural* that the father should marry his daughter, the son his mother, and the sister her brother";† and our colleague quotes, concerning this, a crowd of documents more or less authentic.

Now, in noticing this unfortunate confusion between incest of all

\* The Iroquois, who in 1846 numbered 3,700, appear to have continually married among one another, according to Schoolcraft; and I cannot find any authority for their supposed dislike to consanguineous unions. Only one case of lunacy and one of idiocy are reported among them, and *no case* of blindness or of deaf-dumbness. (Tr.)

† Memoirs of the French Anthropological Society, vol. i, p. 510.

degrees and distant consanguinity, I leave to MM. Devay and Boudin the trouble of agreeing about this universal sentiment: I confine myself to proving that one ought not henceforth to receive any aid from an argument of this nature. The fact is that, according to the time and place, the laws which concern marriage, and in general all the acts of existence, are singularly diverse. To wish to appreciate all these laws at once, and to judge them, for example, according to our modern ideas, is to commit an error into which no clever man would be led, if prejudice did not impel him. History appears to us, if we have not the faculty of transporting ourselves mentally into the midst of past ages—history appears as a monstrosity, the most necessary laws as ridiculous or abominable, and there does not even remain to us the consolation of being able to admire one of the great men who have cast on the world the light of their genius.

I abstain, then, from mentioning this sort of violent accusation which M. Boudin's paper contains against the most admirable nations of antiquity. Supposing that the facts related by our colleague were well established as regards the Persians, the Egyptians, the Phœnicians, etc., we should not be authorised to conclude anything touching the morality of these nations. It is the same about the Greeks, the value of whose social system, thanks to which the integrity of race and duration of families was preserved at Athens as well as in Sparta, M. Boudin has not been able to appreciate. It is quite correct to say that at Athens one *could* marry one's sister; but we know in what horror the Greeks held these incests—a horror of which the tragedies of *Œdipus* and *Phædra* have left us a dramatic memorial.

I hope to present to the Society one day a special paper upon consanguinity among the Greeks; and I will not, therefore, just now correct the historical and philosophical errors made by M. Boudin. Nevertheless, whatever reserve I impose upon myself, I cannot be prevented from denouncing to the Society this system of disparagement—of which the middle ages have left us a tradition—into which our colleague has been drawn. "At Athens as at Sparta," he says, "marriage was only looked upon in a sensual and animal view. Plato himself wishes that alliances between the best of both sexes should be more frequent, and between those who are inferior more rare. Lycurgus puts less ceremony into his opinion; he is astonished that ancient law-givers have not prescribed to man that which is practised among animals, 'finding for their dogs the best bitches, and for their mares the best stallions'." This is how they thought and practised in much-vaunted Greece, which they quote even now as a model of the

highest civilisation." And, to strengthen his quotations, M. Boudin goes back to Plato, in his *State or Republic*, and to Plutarch, in his *Lycurgus*. The quotation from Plato is correct, if we can call a fragmentary quotation correct. But I do not see how we can thence infer that "marriage was only looked upon in a sensual and animal point of view." Who does not know what the "Republic" was? An ideal system, a pure utopia, to which Plato has consigned his ideas upon the government of nations. Now a legislator who wishes to constitute theoretically a fine race, endeavours to find the means for so doing; and this is what Plato points out in the same chapter. "How," says he to Glaucus, "are marriages most advantageous [to the State]? It is for you to tell me. I see that you raise at your house a great many dogs for the chase, and many birds of prey. Do you take care about the breeding, and about pairing them? Among animals of good blood, are there not always some which are superior to the rest?" "Yes." "Do you wish to have all equal, or do you prefer to have the young ones superior to the rest?" "I prefer the latter."\*

This is what Plato *likes best*; and this is what makes M. Boudin consider that at Athens they only considered marriage "in an animal light"! Ah! if M. Boudin had read the whole of the *Republic*, he would soon have perceived that his opinion is not a Platonic one. Besides, if this is the animal point of view, the animal point of view has some good in it. And, if it led to the establishment of a race whose descendants would be the finest and healthiest of men, I should prefer even M. Boudin's "point of view", which ought to be taken without any reference to animality. Does M. Boudin, as a pure spiritualist, desire that children springing from persons of an inferior race should be more numerous than the offspring of the finest and healthiest? Does M. Boudin believe in inheritance?

Let us now come to Lycurgus. And first, let us remark that we only know Lycurgus through Plutarch, his historian; and that, instead of saying "Lycurgus puts less ceremony into his opinion", he ought to refer to Plutarch more or less of this ceremony of Lycurgus. But this reproach is quite a nominal one. A much more serious matter, is the fact that the quotation from Plutarch is incorrect; this may be proved by comparing the above-mentioned passage, quoted by M. Boudin, and the following paragraph, which I take from the *Life of Lycurgus*.† "So it seemed to him that there was something foolish

\* Plato's Works, Aimé Martin's edition, vol. i, p. 97.

† Les vies des hommes illustres. Translatées par Jacques Aymot, vol. i, p. 92. Paris, 1604.

and vain in the laws of other nations respecting marriages; seeing that they put their best bitches and mares to their strongest and best dogs and stallions, by paying and beseeching those who owned the latter, and nevertheless keeping their women under lock and key, for fear they should conceive by others than those who were perhaps insane, unhealthy, or much too old." Thus Lycurgus is not *astonished* that the *ancient law-givers did not prescribe for man as they practised with their animals*. He blames badly assorted and unhealthy marriages; he shows that more is done for animals than for men; and, indeed, where can you find a legislator or a hygienist who looks with a favourable eye on alliances between the *insane*, the *unhealthy*, or the *aged*? and who does not prefer a marriage between young and healthy persons?

Never, perhaps, was marriage more honoured than it was at Athens or at Sparta, from whence comes to us almost all that Western civilisation contains that is either useful or beautiful, even to monogamic marriage. According to Plutarch, it was not possible to hear of an adultery at Sparta; and he must have lost the remembrance of Homeric antiquity, who can dare to speak of it in such scornful language. Allow me to quote to you two passages, one from Homer, the other from Xenophon, and you may well be surprised that any one can have spoken, on the subject of the Grecian race, in an "animal light". When Ulysses, after having been shipwrecked, besought the daughter of the Pheacians, Nausicaa, his eloquent prayer was terminated by a vow, and this vow gives us, as to the ideas the Greeks held concerning marriage, a notion which it would be difficult to reconcile with that given by M. Boudin. "May the gods fulfil thy desires, may they give thee a husband, children, and domestic happiness (*ομοφροσυνην*); for there is nothing in the whole world so touching as to see two persons united in love, who can govern their house with the same spirit."\*

And also in after ages, Xenophon traced in his *Economic* a plan of conjugal life which no society in the world has yet equalled, and which ends with this excellent sentiment: "The most delightful of all joys will be, when you, having become more perfect than myself, shall find in me the most attentive of husbands; when, so far from fearing that age will rob you of my consideration, you will feel, on the contrary, that the more you show yourself a good mistress and manager, so much the more you will see the respect of all the household increase with your years. It is not in this world *beauty* which

\* Homer, *Odyssey*, book vi, 180.

really acquires the right of being respected; it is, indeed, virtues alone.”\*

M. Boudin, in his historical researches, has mercy only on Christians. “The Christian law alone,” he says, “has remedied the evil by forbidding consanguineous marriages up to the fourth degree; that is to say, up to the children of the cousins of first cousins.” If the Christian law *has* remedied the evil, it is the fact, without doubt; but we do not then see the necessity of M. Boudin’s essay. The truth is, that the Roman Church is too wise, too charitable, too conciliating to maintain those prohibitions of another age which had more signification and force in a time of strife and propagandism, but which, at the present day, have no value in Europe. During the first periods of the Christian Church, each converted family dedicated itself to the apostleship, and marriage with strangers was one of the forms of this apostleship. By this means a new Christian nucleus was formed; and the success of these commands was so evident, that they acquired the force of law in the Church. This is easy to prove. St. Augustin, who lived in the fourth century, has clearly stated in the fifteenth book of the *City of God* the causes of this prohibition. He refers to the commencement of creation by one couple; and he says that when men multiplied, marriages between brothers and sisters were prohibited “for a very just reason, that of charity. It was in the most precious interest of mankind,” he adds, “to multiply between them the bonds of affection, and, so far from concentrating these affections on one person, to divide alliances so as to embrace the greatest possible number in the social chain.” And further: “Who can doubt that it is no longer proper to prohibit marriage, even between cousins? And not only for the preceding reasons, for the sake of multiplying friendly relationship, but also because it is a noble instinct of modesty, which, in the presence of those whom relationship ought to make us respect, silences in us those feelings for which we see even conjugal chastity blush.”

These reasons may be peremptory, and, if they were proffered just now, I should have no motive for refuting them; but what have pathology and anthropology to do with them?

In the thirteenth century, Thomas Aquinas, from whom no circumstances could escape which might strengthen canonical prescriptions, enters, on the subject of marriage between relations, into the most trifling details, and nevertheless he does not say a word about the dangers attributed at the present day to these unions. “All persons

\* Xenophon’s Works, i, p. 489, Charpentier’s edition.



who were in the habit of living in the same house were forbidden to marry one another, because if they were able lawfully to have carnal relations together, this liberty would violently inflame their passions; but under the new law, which is the law of the Spirit and of love, several degrees of consanguinity were forbidden, because the worship of God spreads and multiplies by spiritual grace, and not by a carnal origin. Consequently, men must be more debarred from carnal things, so that, attaching themselves to spiritual matters, love may abound in them more and more. This is why, formerly, consanguineous marriage was prevented, except in the most distant degrees of relationship, so that mutual amity might be extended to a larger number by relationship and affinity. They extended it rightly to the seventh degree . . . ; but afterwards the church restrained it as far as the fourth, since it was useless and dangerous to forbid the degrees of consanguinity beyond it."

Will it be thought that the evangelical doctor would have neglected to speak of the diseases which would attack the issue of these unions, if the experience of time had proved to him any facts, or even any affirmative rumours? They quote, it is true, an opinion of Saint Gregory the Great (without giving its source), where it states that "descendants cannot increase from such marriages." (*Experimento didicimus ex tali conjugio sobolem non posse succrescere.*) I do not doubt the correctness of this quotation; but I ask what it means? When they say, *ex tali conjugio*, do they speak of a union between cousins, or of one between father and daughter? M. Boudin ought to have told us this. I am, then, authorised to state that the Roman church, which has been the first to forbid marriages between cousins, has only done so with certain moral ideas, and with this reservation, according to St. Thomas, "that which it is useful to permit at one time, it is advantageous to forbid at another." Now, the civil law has judged wisely, as well as the church itself, that it is in our days useful and necessary to allow that which, in the first days of Catholicism, was always refused. It is only, then, at a very recent time that a few writers have discovered the morbid consequences of marriages between cousins. Which is the first who spoke of it? Our inquiries on this point have not been very fruitful; and the quotations of our opponents are, in general, deprived of all indications of their source. A passage from Foderé has been quoted, which blames marriages between uncle and niece, aunt and nephew, and first cousins, "as tending to debase the race"; if this quotation be correct on this first point, Foderé, who wrote about 1815, would be one of the first

hygienists who described this "tendency"; but in the article, "Marriage," by this author, inserted in the large *Dictionary of Medical Science* (1809), there is not a word on the subject, although the question itself is treated with many details. Esquirol, Rillet, Devay, Chazarain, and Boudin, are then, in France, the chief supporters of the theory of danger in marriages between cousins, which must be considered, whether true or false, as an entirely new one.

#### RECAPITULATION AND CONCLUSION.

1. The question of marriages between blood relations ought to be limited, in anthropology, to the human race, and to refer exclusively to marriages between cousins, uncle and niece, nephew and aunt; since, as to the other degrees of consanguinity, marriage is morally and legally prohibited. This ought to be separated from the question of consanguineous pairing, to which it has only indirect reference.

2. Nobody has answered the objections which I felt it my duty to make in 1862, in a work entitled *Des Dangers attribués aux Mariages entre Consanguins*, although MM. Gourdon, Magne (d'Alfort), Boudin, and Chipault, have since published essays on the subject. These objections still preserve all their force and value.

3. The statistics concerning deaf-mutes, published by MM. Boudin, Brochard, and Chazarain, and those which are attributed, without any proof, to MM. Landes and Perrin, are incorrect in their elements; those of M. Piroux are badly stated. We possess no exact information about the number of consanguineous marriages in France. It is impossible, then, to compare the proportion of these marriages with the proportion of weakly children who are the issue of these marriages. As to the foreign statistics, they are either completely false or exaggerated.

4. As to the very small number of collected individual researches in France, which comprise about sixty or eighty marriages between blood relations, whose issue has been tainted by disease, most of them are wanting in sufficient information for us to be able to judge of the circumstances of the marriages. Some observations are well made, it is true; but we do not see *why* children of consanguineous unions should escape those infirmities which happen to all other children. For six deaf-mutes of the former class, who have been inmates of the Paris asylum, there are three hundred and fifteen who spring from unions between individuals who had no tie of relationship between them.

5. It has not been proved that consanguineous marriages are more

numerous among the aristocratic classes in France and England than among the mass of the population. But even were that proved, nothing will prove that these classes have degenerated; their political situation is altered; rival classes are raised to fortune and honour; but every argument drawn from a comparison between the ancient clans and the citizen class is absolutely valueless; were the aristocratic classes degenerated, a great many causes might be brought forward to account for the fact. As to the relative value of pure races and crossed races, it is a question still requiring study, but closely allied to that of consanguinity. The documents which have been produced up to the present time, seem favourable to the superiority of pure races. In every case it is certain that the French Jews show no special infirmity in their race, and that weakly Jews are not to be found in a larger proportion than weakly Christians. It even appears that they are subject to laws of mortality superior to those of the mixed population among which they live.

6. A number of facts, vastly superior to opposing facts, and in every case facts proving much more, has been collected by MM. Perier, Bourgeois, Lagneau, Voisin, Ferrier, and myself. These facts are either individual or collective. Now, this is to be remarked, that whilst morbid facts laid to the charge of consanguineous unions prove nothing against these unions, because they *may* be due to other causes than consanguinity; isolated or collected facts concerning consanguinity prove, at least, that the supposed dangers have not shown themselves; because if we show places where diseases and consanguineous marriages coexist, nothing will allow us to say that *consanguinity* is the cause. If, on the contrary, we quote places, such as Batz, where these marriages are *the rule*, and where there is no disease, one fact of this nature, properly substantiated for a long period, completely refutes the anti-consanguinity doctrine. Now, these places are very numerous in France and abroad, and I do not doubt but that before long I may be able to increase considerably the list of facts of this nature.

7. The pretended dangers resulting from consanguineous marriages had not been explicitly noticed before the commencement of this century; we find no mention of them among the authors of antiquity. When legislators or theologians have forbidden them, they have done so from excellent motives, referring either to social morals or to domestic order; their interdicts, except for relations in a direct line or of the first degree, have an essentially provisional character; it has, besides, always been allowable to remove these interdicts. This

custom is usual in these days; the civil law makes no mention of it in any country.

8. Consequently, in the present state of science and in a physiological point of view, we are not authorised to blame marriages between first cousins: *it is a question* to be discovered whether it may be useful to recommend them, now that the dispersion of families makes moral and domestic conditions so different from what they formerly were; for if, on the one hand, everything tends to make us believe that *healthy* consanguinity is favourable to the offspring, it *may be* that morbid consanguinity would be unfavourable to them.

## APPENDIX \*

DOCTOR ANCELON communicated to the *Académie des Sciences*, on the 18th of January, some remarks on the value of statistics as applied to consanguineous marriages. There has been, no doubt, a great deal of discussion lately on this subject. To what purpose? Are we favoured, now-a-days, with some new social reason? Are these marriages now more frequent than in former days? That which astonishes us is not the number of evils imputed to consanguineous marriages, but the enormous quantity of these marriages which have been noticed since the subject has been mooted; and above all, the lengthened observations which, they say, have been made upon them. Is it not surprising, for example, that all of a sudden, in a small rural district of La Meurthe, fifty-four consanguineous marriages have been met with and examined, with the following consequences:—

1. Marriages which have been sterile	-	-	14
2. Marriages whose issue has died before the age of puberty	-	-	7
3. Marriages which have produced children afflicted with scrofula, tubercles, deaf-muteness, etc.	-	-	18
4. Marriages whose issue has required no particular observation	-	-	15
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54			

What must we infer from this? Assuredly these data would be very alarming if we could only look at them from one point of view, and neglect the multiplicity of causes of degeneracy introduced into society since the end of the last century. But the registrars of sta-

\* Dr. Dally, the learned author of this memoir, has been good enough to forward me the following report of a paper on the same subject, read before the Academy on the 18th January. It is so valuable, that I have not hesitated to translate the whole of it. (Tr.)

tistics have, perhaps, hardly considered what would become of their statistical display if the question is reconsidered. Are they uneasy as to what they would discover in examining non-consanguineous marriages? While waiting until, if possible, a statement of consanguineous marriages, contracted anterior to 1800, be made, we are called upon to examine contemporaneous non-consanguineous marriages, of which we have here the results.

Dieuze, with a population of 3,700 souls, can count only four consanguineous marriages, the consequences of which we will examine farther on; as to the non-consanguineous marriages, they are analysed in the following manner:—

1. Sterile marriages	-	-	7.50 per cent.
2. Marriages whose issue is scrofulous, deaf and dumb, etc.	-	-	47.33 „
3. Marriages whose issue has died before the age of puberty	-	-	0.69 „
4. Marriages which have given rise to no particular observations	-	-	44.93 „

The balance here is not favourable to non-consanguineous marriages; and that nothing may be wanting in our manner of proof, let us examine our four consanguineous marriages. The first of these marriages, between first cousins, dating some thirty odd years ago, has remained sterile. The three others, which have also been between first cousins, came from the same stock. From the first consanguineous marriage there were born five children—three boys and two girls. The eldest of the boys married his first cousin, who has borne him two healthy children: the second, aged twenty-five, is still a bachelor; the third died of epilepsy at the age of twenty. As to the youngest daughter, married to her first cousin a little before her eldest sister, she has already three healthy children. Except the epileptic patient, whom we mentioned above, all the other members of this numerous consanguineous family have enjoyed the most flourishing health up to this time, with the exception of two, who have died of acute pneumonia.

After all this, and until we obtain the double series of statistics of which we have just given a specimen, we believe we have a right to conclude that we must search elsewhere for the causes of the degeneracy with which some people endeavour to charge consanguineous marriages.

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